

Interactive comment on “Effects of climate variability on Savannah fire regimes in West Africa” by E. T. N’Datchoh et al.

E. T. N’Datchoh et al.

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Answer to comment on the manuscript entitled “Effects of climate variability on Savannah fire regimes in West Africa” by E. T. N’Datchoh et al.

We are grateful to you for your interest, your contribution and your pertinent observations that help us improve our work. The entire manuscript has been rewritten taking account the divers referees comments and observations. Before the final submission, it will be corrected by native English speaker for language skill improvement.

1) General comment on the quality of the English spelling :

The first manuscript will be edited by a native scientific English speaker before the last version submission.

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2) The references :

In the light of the referee comments and suggestions, we are in the process of reinforcing bibliography for the entire manuscript.

3) Bias on the data : Thirdly, and most importantly, the authors do a very poor job of discussing the bias and error inherent in the burned area product they use in the study. This so called “low resolution” bias has been well documented in the literature (See Boschetti et al 2004 and Laris 2005). At the very least the authors should carefully explain the bias and note in the discussion how it may (or may not) influence their findings.

We agree with your comment that we poorly discuss the bias and error inherent in the burned area product used in our study. The mentioned author, Boschetti's contributions on that issue, will be taken into consideration. The main bias of satellites fire product is the existence of large discrepancies between the different satellite based products (Boschetti et al., 2004; Laris 2005; Roy and Boschetti, 2009; Chang and Song, 2009; Giglio et al., 2010). One of the main biases of these data is the underestimation of burned area over Africa thus West Africa which may have some impact the correlation coefficients with large scale climatic factors. In the case of the present study it appears difficult to conclude on the potential impact of these biases. One of the main shortcomings of the underestimation may be a weak representation of the link between burned areas and these climatic factors.

Also, for the purposes of mapping fire in African savannas, 1km x 1km data should be considered “coarse” resolution given that fires burn fine-scale mosaic patterns in many environments.

From our viewpoint, although this 1 km x 1 km spatial resolution can appear from an ecological perspective as coarse resolution, it is from a climatic view, mainly in term of relationship between climate factors and fire at the regional scale, a high resolution scale, notably when we compare to other products and modelling results (from 25 km

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x 25 km to 1o x1o resolution for example).

4) On the link climate-fire

Fourthly, the explanation for how the regional climate factors influence fire regimes is confusing. This may be due in part to the poor English. Regional climate factors influence precipitation, not fire. Fires are set by people for a wide variety of purposes and the time and place they set fires (and how those fires spread) may be indirectly related to climate factors. This should be stated clearly.

We agree that regional climate factors have direct impact on precipitation but also air temperature which is important during the ignition phase. However precipitations impact biomass availability which is the fuel for biomass fire. Therefore, by non-linear relationship, the regional climate factors have some relationship with biomass burning. We will make sure that we explain this point in our revised manuscript.

5) Table of fire regions

I think that the addition of a table that indicates the different fire “regions” (not sources which is a poor choice of words) studied listing the expected influences of climate on precipitation and how this, in turn, impacts vegetation growth and fire would be extremely informative. In its current organization I found it difficult to interpret the results.

This point has been made clear in our revised version, by replacing sources with regions.

6) Climate, fire and human livelihood and burning practices

Finally, the authors make several point about how in some areas fire increases following a low rainfall year and in others fire increases after a high rainfall year. This should be carefully explained in the text (it is not) as it is a critical point. Also, the authors should reference studies indicating an important difference between rainfall determined and fire determined savannas in Africa (See Sankaran et al 2005) for an explanation. It is important to explain why fire patterns change as a function of precipitation in different

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types of savannah. It would also be useful to add some information on human livelihood and burning practices in these different regions as fire uses vary by region (See work by Mbow et al). It must not be forgotten that these patterns are ultimately shaped by human land use practices (in more arid savannas, the goal is to prevent all fire, in moister savannas, the goal is to time fire appropriately so as to take advantages of its impacts on vegetation such as regeneration of perennial grasses (see Mbow et al).

This observation will be taken into consideration in the revised version. Rainfall, human impact as well as referenced literature will be introduced as well.

Few additional comments and observations:

7) Influence of large scale factors

I found this statement interesting, “This study revealed that, even though fire during the dry season was a purely anthropic activity, the variability of BA could be influenced by physical phenomena such as sea surface temperatures (SST) and sea level pressure (SLP) via their link to the climate.” Although the authors stress the variability in area burned they find in their data, the data also shows a striking pattern of regularity in terms of the timing or seasonality of the fires. The onset and peak fire points seem very consistent over the study period. This seems to warrant at least a comment in the discussion.

Although the onset and peaks fire points seem uniform, there is a variability at inter-annual and intra-annual scale since burned areas extent varied from one season to another and also in the same season. From year to year the total burned areas varies and also into the same season, from a month to another these burned areas amount varied which allowed us to use the variability term. Therefore, fire practices by population are independent to climatic conditions, the onset occurs at the same period of the year as well as the peak, the climatic conditions only affect fire magnitude and intensity (in relation with the available biomass and surface air temperature).

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8) Interannual variability

The authors explain the 2000-2001 anomaly in burned area but not the 2004-2005 one which is the most dramatic. Can it too be explained? Why not? This sentence seems half true, “Even if fires are an essential anthropic activity in this region, the climate regulates the quantity of available dry combustible and its state (ground and combustible humidity, air temperature, wind speed, etc.) for fires to spread.” This is only half true because people regulate the timing of setting fires which can determine the humidity, wind speed, air temperature and conditions of the vegetation when a fire starts.

The 2000-2001 anomaly in northern African regions has been referenced in the literature (Le Page et al., 2008) in opposition to the minimum observed during the season 2004-2005, where we did not find references. This minimum is remaining an interesting research question which can be investigate through future work.

9) Correction on the English spelling

I am not prepared to site specific grammar errors as there are too many. However, there are some key phrases or words that are confusing and that should be addressed:

1. “Bush fires.” I would stay away from this term as it has little scientific meaning. If these are “savanna fires”, then use that term. “Biomass burning” is also a possibility, although it usually has a broader meaning.

This view will be amended in the revised version of the manuscript.

2. Although I agree that the fires are “controlled” to some degree in West Africa, this does not seem appropriate language. Just state “fires were observed” is better.

We perfectly agree with your comment.

3. I associate “sources” with smoke, not fire. I would call these “regions” or “areas”.

We perfectly agree with your comment.

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4. El Nino events do not necessarily lead to changes in fire “practices” they may result in changes in burned area.

We will take this remark into consideration in the revised version

5. The phrase “variability in the span of BA” makes no sense. What is a “span” of burned area?

The term “span” is used here for burned areas extent. But it does not seem to be the appropriate term.

6. I would use “human” rather than anthropic

Ok

7. I do not agree that the results “reveal an evolution” in Burned Area. Change, perhaps or variation over time.

Ok

8. “Vulnerability map” seems to be an odd term, as it is usually associated with accidental fire. African fires are often purposefully lit.

We used this term because some pixels are more exposed to fire than some others. The “vulnerability” term is then used in reference to ecosystems (the threat to their resilience to repeated fire).

Interactive comment on Earth Syst. Dynam. Discuss., 3, 1021, 2012.

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